N42237.AR.000404
NSB KINGS BAY
5090.3a

LETTER FROM NAVY REQUESTING A TIME EXTENSION FOR SUBMISSION OF CORRECTIVE ACTION PLAN AND INTERIM MEASURES REPORT FOR SITE 11 NSB KINGS BAY GA

3/20/1996

NSB KINGS BAY

0141

31547.000
09.01.00.0110

5090
Ser FE4\0972
20 MAR 1996

CERTIFIED MAIL
RETURN RECEIPT REQUESTED

Georgia Department of Natural Resources
Environmental Protection Division
Attn: Mr. Billy Hendricks
Hazardous Waste Management Branch
205 Butler Street, S.E.
Floyd Towers East, Suite 1154
Atlanta, Ga 30334

Dear Mr. Hendricks:

This references our letter 5090 Ser FE4/7207 of December 21, 1995 proposing a due date of March 15, 1996 for the Interim Measure Report for Site 11 (the old Camden County Landfill). However, we would like to request a time extension until May 01, 1996 for submission of the same report. This letter is dated after March 15, 1996 but was faxed undated to your office on March 15, 1996 to meet the required due date.

As discussed during the March 07, 1996 telephone conversation between your Mr. Billy Hendricks and SUBASE's Mr. Sandi Mukherjee, this time extension will allow us to prepare a more complete submittal to your office by incorporating the recent findings of United States Geological Survey (USGS) and the comments provided by USGS and the Navy on the draft version of the subject report. A copy of the comments provided by USGS are attached as enclosure (1), and the Navy's as enclosure (2).

5090
Ser FE4\

Please address all correspondence to "Commanding Officer, 1063 USS Tennessee Avenue, Naval Submarine Base, Kings Bay, GA 31547-2606." SUBASE Kings Bay point of contact on this matter is Mr. Sandi Mukherjee, (912) 673-2001, extension 1217. Thank you again for your continuing support and cooperation as we move ahead in resolving this problem.

Sincerely,

JOHN R. GARNER
Leader, Environmental Division
By direction of the
Commanding Officer

Encl:
(1) USGS comments on Interim Measure Report for Site 11
(2) Navy comments on Interim Measure Report for Site 11

Copy to:
Anthony Robinson, SOUTHNAVFACENGCOM
David Hicks, United States Geological Survey
Ted Taylor, ABB-ES

UNITED STATES GEOLOGICAL SURVEY

Georgia District
3039 Amwiler Rd.
Suite130, Peachtree Business Center
Atlanta, GA 30360-2824

Telephone Number: *770-903-9100*
Telefax Number: *770-903-9199*

TO: SANDY MUKHERJEE

FROM: ELLIOTT JONES

DATE: 3/19/96

MESSAGE Sandy,

Enclosed is the most recent revision of my letter reviewing the 1M Addendum draft. I am also sending my 4/94 water-level contour map, which is the first of two enclosures mentioned in the letter.

-Elliott

NUMBER OF PAGES TO FOLLOW: 8

United States Department of the Interior

GEOLOGICAL SURVEY
Water Resources Division
Peachtree Business Center, Suite 130
3039 Amwiler Road
Atlanta, Georgia 30360-2824

March 1, 1996
(amended March 19, 1996)

Anthony B. Robinson, Engineer-in-Charge
Department of Navy, Southern Division
Naval Facilities Engineering Command
2155 Eagle Drive
North Charleston, SC. 29418

Dear Anthony,

This letter summarizes our review of Interim Measure Phase I Activities: Evaluation and Recommendations Report Addendum, Site 11, Old Camden County Landfill, Naval Submarine Base, Kings Bay, Georgia. The version of the report we reviewed is the Preliminary Draft version, labelled 'Working Draft' on grey page banner, and dated February, 1996. It was prepared by ABB Environmental Services, Inc., for the Navy, SouthDiv, who asked the USGS to review it. Enclosed you will find the annotated manuscript we reviewed.

Following are page-numbered comments referring to a particular passage of text or some aspect of the figures and tables that warranted more than a margin note. These comments are ordered under the major section headings of the report. The point in question is indicated on the appropriate page of the manuscript by an encircled * in the margin. If more than one passage on a given page elicited comment, they are distinguished by lower-case letters in parentheses on both the page and in this letter. In this review, little attention was given typographical accuracy or editorial style—thus, the margin notes in the manuscript and on the figures are technically substantive, and should be given attention.

Comments (by section and page):

Executive Summary

p. i. There was no Executive Summary in my version (blank page).

1.0 Introduction

p. 1-1. Is this addendum intended to be submitted in addition to a re-submission of the November 1994 IM report, which received the NOD from GA-EPD? If so, then the earlier report will require substantial revision as per previous USGS comments and GA-EPD comments submitted with NOD prior to re-submission.

1.2 Objectives of IM

p. 1-2. The stated overall objective of the IM is 'to hydraulically control movement of the most contaminated portions of the VOC plume within the surficial aquifer using a GWE system.'

12:32 pm, March 19, 1996
/lej/Memos/960319.robinson

At the meeting with GA-EPD on February 15, 1996, Billy Hendricks indicated that the end objective of the clean-up activities at Site 11 should be to extract completely all contaminants from the landfill, including recovery of contaminants that have migrated off-site. To clarify that this IM is, as stated, an interim and not a final measure, it might be helpful to discuss the ultimate objectives of the clean-up activities, which will be addressed in the final Corrective Action Plan.

p. 1-3a. The stated objective of the first phase of the interim measure was 'to collect site-specific data to support long-term corrective measures'. This is the sort of information GA-EPD wants to see. This point should be expanded using explicit statements that describe precisely what site-specific data were collected (or are being collected) and exactly how they were (will be) used to plan long-term corrective measures.

1.3 Purpose of Addendum

p 1-3b. In our opinion, this addendum does not 'provide a more concise overview of the process used to evaluate the performance of the IM Phase I GWE system' as stated. If the intention is that the three capture-zone analyses are used to evaluate the system, it should be stated here, and reiterated at the introduction of that subsection. Also, evaluation of the effectiveness of the system is an ongoing process, and will be supported by other types of analyses, in addition to the capture-zone analyses.

p. 1-4. The evaluation of the effectiveness of the GWE system is the primary topic of the IM report and this addendum. Rather than introducing this point at the end of a list of additional issues addressed, it should be stated at the beginning of this section along with a statement of the approach that has been and will be used to arrive at a convincing evaluation. The GA-EPD needs to know how the effectiveness of the system is to be evaluated.

p. 1-5(a). USGS analysis of continuous water-level data indicated that nearly 100% of the drawdown due to pumping occurred within 600 minutes. Further comments in Section 2.3.

p. 1-5(b). The capture-zone analyses are inconclusive as presented because of incomplete hydrogeologic characterization. The three approaches to capture-zone delineation depend on the assumption that ground-water flow at Site 11 is more-or-less uniform from east to the west. If ABB-ES elects to include the capture-zone analyses, they should be qualified in such a way that a different assessment of the ground-water-flow system would not discredit the analysis. We suggest a definitive statement of the purpose and usefulness of these analyses be included when they are first introduced. That is to say, the capture-zone analyses are useful for approximating the effect of the IM Phase I GWE system on the ground-water flow system. Due to the many inherent uncertainties associated with these analyses (see comments on section 2.3 below), the USGS suggests that the capture-zone analyses could be improved by addressing the comments on Section 2.3, and that additional methods should also be used to evaluate the overall effectiveness of the IM Phase I GWE system.

2.0 Data Assessment

2.1 Ground-Water Level Trend Analysis

p. 2-2. The discussion of the numerous hydrographs is insufficient to justify their inclusion in the report. In the following comments there are several suggestions on how specific example hydrographs can be used with appropriate annotation to strengthen various points of discussion. The hydrographs, in their entirety, could be presented in the SRFI report. Why is there more than one hydrograph for each well? Do the vertical dashed lines on the hydrograph represent pumping phases? The 20-foot scale on the ordinate (water-level axis) is too large to show accurately water-level fluctuations that typically are less than two feet during the period shown. None of the effects discussed later (tidal, recharge/discharge, and barometric), however minimal, could be discerned at this scale.

p. 2-7(a). To evaluate the barometric efficiency, it would be best to choose a time period when barometric-pressure change is likely to be the only factor influencing ground-water level fluctuations, though it is, of course, impossible to remove the effect of ground-water recession. The period chosen for this analysis coincides with large and sudden changes in pumping stress due to the start-up of the recovery wells. As a result, it would be difficult to separate the unknown effects of the pumping change from the unknown effects of barometric pressure during this period.

p. 2-7(b). The discussion of change in ground-water levels in response to rainfall events needs to be substantiated with a figure including a hydrograph and rainfall data plotted on the same time scale. All points of discussion included in the text should be indicated on the figure. It is difficult to distinguish any difference between recession following ('within one to two weeks') a peak due to a rainfall event and the resumption of regional recession trend. The last sentence, beginning 'Less significant rainfall...', is hard to understand—what is a 'more natural decline', and how can rainfall 'reestablish the regional recession trend'?

2.2 East Boundary Flow

p. 2-8. Placing a constant-head boundary at some point east of the landfill to drive ground-water flow westward is a conservative modeling approach if it is known with certainty that contaminants originate solely from the isolated 'hot spots' at the western boundary of the landfill and migrate westward toward the subdivision. If, for example, there are other undetected sources of contamination at other locations within the boundaries of the landfill and there is a ground-water mound beneath it, the digital model presented in this report could not accurately predict the movement of those contaminants, which could be emanating from the site in other directions. All three approaches used in the capture-zone analysis are based on the assumption that the natural flow of ground water is uniform from east to west across the landfill. Again, this discussion should be qualified in such a way that a different assessment of the ground-water-flow system would not discredit the analysis. The first sentence of this paragraph is a clear statement of fact that may be incorrect. It would be more accurate to begin the paragraph, 'Based on available data, it is assumed that groundwater beneath Site 11 flows westward...'. This would allow for refinement of the conceptual ground-water-flow model should a ground-water mound be detected based on additional data from areas beyond the landfill.

2.3 Capture Zone Analysis

p. 2-8b. It would be helpful to reiterate that the purpose of the capture-zone analyses is to provide a preliminary evaluation of the effectiveness of the IM Phase I GWE system (see comment p. 1-3b), and to describe briefly how results of these analyses support the objectives of the IM.

p. 2-10. Figure 2-1(b) indicates a uniform ground-water gradient (uniform flow) from right to left. No consideration is given to the possibility of a recharge mound or ground-water divide.

p. 2-11. The 'length' of the capture zone, *L*, is questionable, especially considering the possibility of other ground-water-flow-system scenarios. Also, some discussion of the measurement or estimation of runoff and evapotranspiration is warranted because net infiltration, *p*, is dependent on them. Uncertainty in the values of *L* and *p*, which are both inversely proportional to the width of the capture zone, the desired result, casts doubt on the credibility of this analysis.

p. 2-16. The graph in Appendix D requires further explanation. Why are numerous points plotted on the graph at different times for the same well? If the recession factor for a given well is a constant, there is no need to plot it numerous times for the same well. Do the periods represented by the numerous points for each well actually indicate the time periods for which recession was analyzed for a given well? Also, the discussion in the text indicates that three wells were chosen as control wells, representing the three aquifer units. Were the three regional recession factors that were determined for these control wells also applied to each of the other wells in their respective unit? If so, why are different values of the recession factor presented in the graph for each well. Additionally, values of the recession factors for control wells 11-6, 11-18, and 11-11C appear to change over time on the graph for some of these wells and remain constant for others. The factor for well 11-6 changes from 0.024 to 0.032 to 0.026; for well 11-18 it appears to be a constant 0.031; and for well 11-11C from 0.028 to 0.045 to 0.035 to 0.033. What is the explanation for these variations, and why do these values not match the range given in the text? It seems the analysis of trend has been made more complicated than necessary by this graph, or, perhaps, salient points have not been adequately conveyed in the discussion. A graphical depiction of recession trends and slopes superimposed on a representative hydrograph would help clarify this discussion.

p. 2-17(a). There is not adequate justification given for the choice of April 4, 1994, water levels as the static/reference water level used to calculate drawdown, nor is there adequate discussion of the data to state that water levels in most wells 'had not fully recovered' from, I assume, Stage 2 pumping. Stage 2 pumping totalled only about 15 gpm, including only about 6 gpm from the intermediate and deep zones. Judging from the rapid water-level responses observed at Site 11 since October 1995, when USGS recorders were installed, it is likely that the water level in any of the monitoring wells would have recovered fully in a few hours from this small pumping stress. It would be helpful to the regulators to present a detailed hydrograph of this time period showing water-level fluctuations in one monitoring well that is close to the recovery wells in support of this hypothesis. If correct, it should be a simple matter to illustrate on such a hydrograph(s).

p. 2-17(b). USGS analysis of the continuous water-level data indicated that virtually 100% of the drawdown due to pumping occurred within 600 minutes of the initiation of pumping. The stated time period (9,000 minutes) to reach steady-state conditions and the percentages of total drawdown due to pumping that are stated to have been achieved within the first 12 hours seem incorrect. Regardless, all stated values are presented without adequate discussion. For these values to be convincing, they should be supported by selected example hydrographs, including appropriate annotation.

p. 2-18. Plotting the location of every monitoring well on Figure 2-2 is unintentionally misleading, because it implies that water-level measurements for each well were used in the construction of this water-level surface. Since a small subset of these wells was used to generate the potentiometric surface shown, only the pertinent data should be plotted in this figure, not all the unused well symbols and IDs, which are a distraction. If it is necessary to indicate to the reader the locations of all the monitoring wells, it could be done in another figure. Also, enclosed for consideration, is a different interpretation of this potentiometric surface based on the data in the Supplemental RFI Report.

p. 2-19a. Again, a representative hydrograph could lend support to the claim that 'steady-state conditions were not established in the shallow unit'. The statement 'water levels within this unit continue to drop throughout the course of the test' seems irrelevant. In the absence of rainfall, water levels in all surficial-aquifer wells drop continuously due to natural ground-water recession. Declining water levels in the shallow unit do not necessarily indicate 'drainage...to underlying units', if all the water levels fall naturally over time.

p. 2-19b. In the middle of the page is the statement 'interpretive distance drawdown plots were generated...'. There is insufficient discussion of these distance-drawdown plots, how they were generated, and how they were used in the generation of figure 2-3. The observation wells used in the distance-drawdown plots are located in various radial directions with respect to the centers at the recovery wells. Some of these directions are away from the other recovery wells, and some of these directions are toward the other recovery wells. How is the compounded drawdown effect of the neighboring recovery wells considered in the construction of the distance-drawdown plots? How are the plots used to augment the adjusted drawdown measurements used to generate figure 2-3? The adjusted drawdown measurements should be indicated in figure 2-3. Also, something should be indicated on figure 2-3 to illustrate the connection between the distance-drawdown plots and the drawdown surface generated.

p. 2-22. The process of superimposing water-level and drawdown maps to create a new water-level surface is difficult to comprehend as described, and problematic. Firstly, figures 2-2 and 2-3, which were used to create figure 2-4, are presented without data values, making it difficult to evaluate their accuracy. A figure similar to 2-2 presented in the SRFI report does indicate 16 measured data points for the same time period; however, the data do not justify the curvature in contour lines (which are not exactly the same as in figure 2-2 of this report). As mentioned in comment for p. 2-18 above, I will enclose in this letter another interpretation of the data. Secondly, both figures 2-2 and 2-3 are based on a relatively small number of actual measured data points (16 in the case of figure 2-2), so there is a degree of uncertainty associated with the location of the contour lines. There also is uncertainty related to the distance-drawdown analysis used in the construction of figure 2-3 (see comment on p. 2-19b), in addition to the uncertainty in the estimation of the corrected drawdown values (see comments on pp. 2-16, 2-17(a), 2-17(b)). Superimposing the two figures compounds all this uncertainty. The numerous 'equipotential data points' in figure 2-4 are artificial values. Thus, the statement that the process 'provides additional equipotential data for better resolution, additional equipotential data points between wells, within the area surrounding the recovery wells' is incorrect and misleading. The composite surface is subject to greater uncertainty than either of the component surfaces.

Considering all the uncertainty associated with each of the several analyses used in the construction of figure 2-4, the flow paths drawn based on the equipotential contours are of limited accuracy, and are not a convincing depiction of the flow system nor a reliable indication of the capture zone of the recovery system. Because of its limited usefulness, we suggest this analysis be removed from the report and eventually replaced by a more accurate representation of water levels and drawdown based on field measurements and not encumbered by the inaccuracies of several levels of interpretation. If it is to be retained, a thorough discussion of uncertainty should be included in the discussion of each of the several analyses that precede and form a basis for this superimposition procedure. Additionally, a summary discussion of all sources of uncertainty should be included here.

p. 2-23. The apparent data values plotted to the nearest 0.01 ft on figure 2-4 are not identified. If they are measured water levels, nine of the 13 values do not fit the contour lines. Also, the values do not agree with data presented in the SRFI report for the same time period. This figure, if it is to be retained in the report, needs substantial revision to make it legible and understandable.

p. 2-27. What is the basis for the location and elevation of the specified head boundaries at both the eastern and western boundaries? What is the basis of the configuration of the base of the aquifer? How were the various rates of recharge distributed throughout the model domain, and how were these distributions determined? Further discussion of infiltration and evapotranspiration values is warranted. Overall, this is a cursory discussion of digital model input parameters—almost all the information is stated in table 2-4. This discussion should provide the reader with some additional insight into how the values listed in table 2-4 were determined. As written, it provides little information that is not in table 2-4.

p. 2-32. Why is the simulated drawdown shown in figure 2-8 much greater than the observed drawdown in figure 2-3? This large discrepancy between simulated and observed values indicates poor calibration of the model under stressed conditions, and casts doubt on the credibility of the digital model analysis.

3. Conclusions and Recommendations

p. 3-1. Using values of transmissivity from aquifer tests and well construction information, USGS computed an expected specific capacity for the recovery wells. Comparing the expected to the actual specific capacity of the wells indicated a very low well efficiency.

p. 3-4. The additional three simulation scenarios of the FLOWPATH model are subject to the same limitations stated in the comments on pp. 2-8, 2-27, and 2-32, above. Considering the uncertainty of boundary conditions and model parameters, and the apparent lack of model calibration, the accuracy of results from these analyses of the upgraded IM system is questionable.

p. 3-5. It is difficult to discern the locations of the recovery wells at this scale. All monitoring wells and piezometers should not be shown on this figure when the focus of the discussion is the recovery wells. Resolution could be enhanced by increasing the scale (zooming in), and displaying only the recovery wells.

p. 3-6(a). Again, it is doubtful that the FLOWPATH model is accurate enough to be used as a predictive tool. Even if it were free of the problems previously stated (see comments on p. 2-27 and p. 2-32 above), the model would still be subject to the limitations of the entire capture-zone analysis, which are discussed in the comment for p. 1-5(b) above.

p. 3-6(b). Considering the comparison of figure 2-3 and 2-8 (see comment for p. 2-32 above), figure 3-4 also most likely substantially overestimates drawdown.

p. 3-6(c). What is meant by the statement 'may not be hydraulically influenced by the existing wells?' It is clear from the measured drawdown maps that the pumping provides some drawdown in all the proposed well locations.

p. 3-7, p. 3-8. See comments on p. 3-5 above.

p. 3-10(a). The IM Phase I GWE system was intended to prevent migration of contaminants across the western boundary of the landfill. Pursuant to GA-EPD's statements on its ultimate objectives, the focus of future clean-up efforts may eventually be to develop techniques or procedures that will reverse ground-water flow in the Crooked River Plantation subdivision, including, but not limited to, the installation of an impermeable cap over the landfill.

p. 3-10(b). Where will the performance-monitoring wells be located? How was the number, three, determined? How were the locations determined? More than three are probably warranted given the uncertainty of the analysis of ground-water-flow directions.

I hope this review has been constructive. If you have any questions, please feel free to call.

Sincerely,

L. Elliott Jones

L. Elliott Jones
Hydrologist

Enclosures: Water-level contour map, April, 1994; annotated manuscript (without appendices).

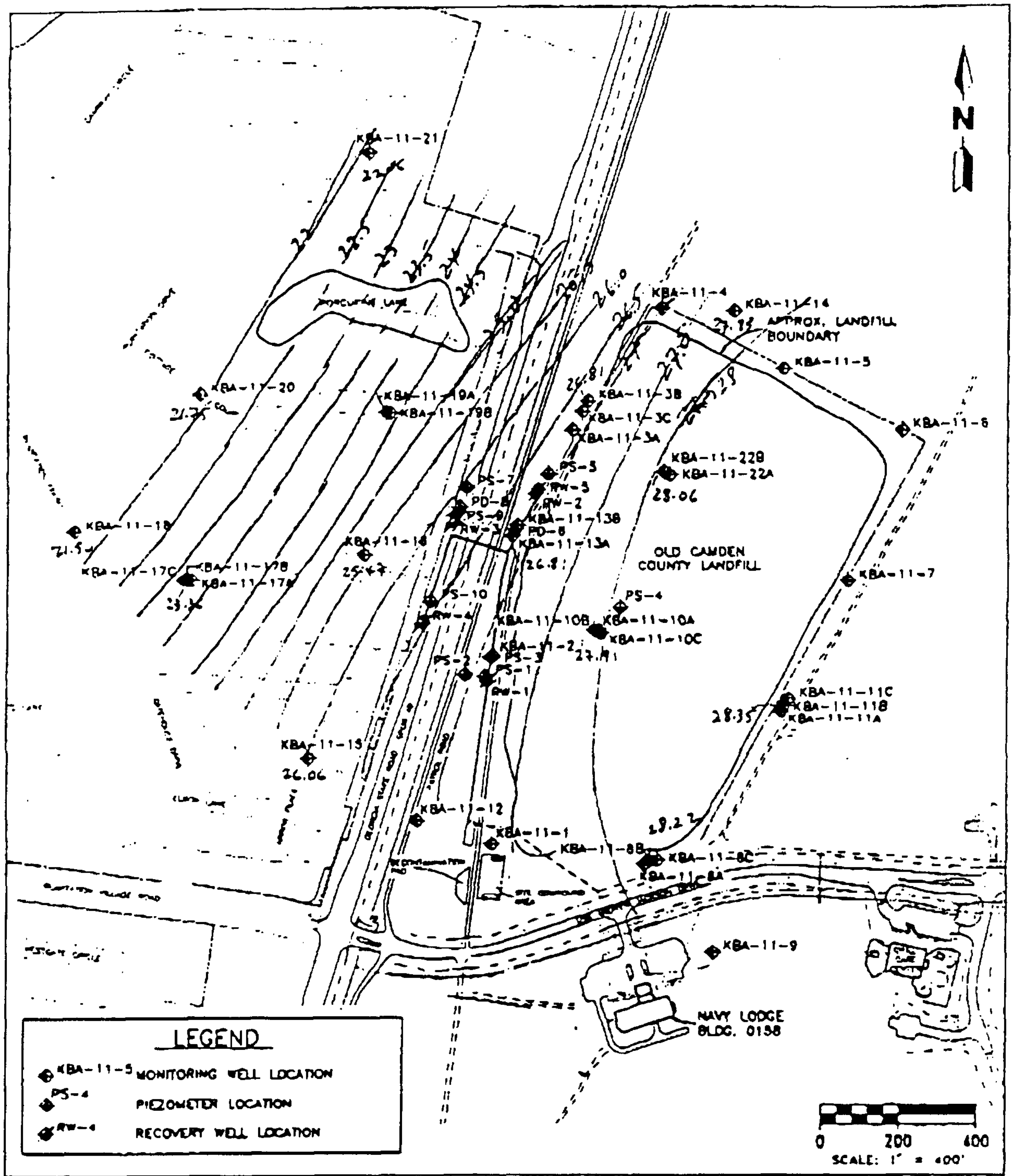

FIGURE 2-1

MONITORING WELL, PIEZOMETER AND RECOVERY WELL LOCATIONS

IM PHASE I ACTIVITIES EVALUATIONS AND RECOMMENDATIONS REPORT, SITE 11
OLD CAMDEN COUNTY LANDFILL

NAVAL SUBMARINE BASE
KINGS BAY, GEORGIA

O: DWG:488\08503-05\IM_PHASI\FIG_2-1\DWF\941117

WATER LEVELS, APRIL, 1994

KB IM I-EVAL&RECI__
rev. 11/94

2-2

Contours drawn by L. Elliott Jones USGS
3/5/96

UNITED STATES GEOLOGICAL SURVEY

Georgia District
3039 Amwiler Rd.
Suite130, Peachtree Business Center
Atlanta, GA 30360-2824

Telephone Number: *770-903-9100*
Telefax Number: *770-903-9199*

TO: SANDY TRUETT (912) 673-8875

FROM: ELLIOTT JONES

DATE: 3/14/96

MESSAGE Sandy,

Sandi Mukherjee asked me to fax you this letter today, so that it could accompany your request to GA-EPD for an extension of the deadline for submission of the subject report. I should mention that the enclosures are not included, due to their volume. The annotated report was sent to ABB-ES in Knoxville on March 1. Let me know if you need anything else.

— Elliott

NUMBER OF PAGES TO FOLLOW: 7

United States Department of the Interior

GEOLOGICAL SURVEY
Water Resources Division
Peachtree Business Center, Suite 130
3039 Amwiler Road
Atlanta, Georgia 30360-2824

March 1, 1996
(amended March 14, 1996)

Anthony B. Robinson, Engineer-in-Charge
Department of Navy, Southern Division
Naval Facilities Engineering Command
2155 Eagle Drive
North Charleston, SC. 29418

Dear Anthony,

This letter summarizes our review of Interim Measure Phase I Activities: Evaluation and Recommendations Report Addendum, Site 11, Old Camden County Landfill, Naval Submarine Base, Kings Bay, Georgia. The version of the report we reviewed is the Preliminary Draft version, labelled 'Working Draft' on grey page banner, and dated February, 1996. It was prepared by ABB Environmental Services, Inc., for the Navy, SouthDiv, who asked the USGS to review it. Enclosed you will find the annotated manuscript we reviewed.

Following are page-numbered comments referring to a particular passage of text or some aspect of the figures and tables that warranted more than a margin note. These comments are ordered under the major section headings of the report. The point in question is indicated on the appropriate page of the manuscript by an encircled * in the margin. If more than one passage on a given page elicited comment, they are distinguished by lower-case letters in parentheses on both the page and in this letter. In this review, little attention was given typographical accuracy or editorial style—thus, the margin notes in the manuscript and on the figures are technically substantive, and should be given attention.

Comments (by section and page):

Executive Summary

p. i. There was no Executive Summary in my version (blank page).

1.0 Introduction

p. 1-1. Is this addendum intended to be submitted in addition to a re-submission of the November 1994 IM report, which received the NOD from GA-EPD? If so, then the earlier report will require substantial revision as per previous USGS comments and GA-EPD comments submitted with NOD prior to re-submission.

1.2 Objectives of IM

p. 1-2. The stated overall objective of the IM is 'to hydraulically control movement of the most contaminated portions of the VOC plume within the surficial aquifer using a GWE system.'

Encl (1)

At the meeting with GA-EPD on February 15, 1996, Billy Hendricks indicated that the end objective of the clean-up activities at Site 11 should be to extract completely all contaminants from the landfill, including recovery of contaminants that have migrated off-site. To clarify that this IM is, as stated, an interim and not a final measure, it might be helpful to discuss the ultimate objectives of the clean-up activities, which will be addressed in the final Corrective Action Plan.

p. 1-3a. The stated objective of the first phase of the interim measure was 'to collect site-specific data to support long-term corrective measures'. This is the sort of information GA-EPD wants to see. This point should be expanded using explicit statements that describe precisely what site-specific data were collected (or are being collected) and exactly how they were (will be) used to plan long-term corrective measures.

1.3 Purpose of Addendum

p 1-3b. In our opinion, this addendum does not 'provide a more concise overview of the process used to evaluate the performance of the IM Phase I GWE system' as stated. If the intention is that the three capture-zone analyses are used to evaluate the system, it should be stated here, and reiterated at the introduction of that subsection. Also, evaluation of the effectiveness of the system is an ongoing process, and will be supported by other types of analyses, in addition to the capture-zone analyses.

p. 1-4. The evaluation of the effectiveness of the GWE system is the primary topic of the IM report and this addendum. Rather than introducing this point at the end of a list of additional issues addressed, it should be clearly stated at the beginning of this section along with a precise statement of the approach that has been and will be used to arrive at a convincing evaluation. The GA-EPD needs to know how the effectiveness of the system is to be evaluated.

p. 1-5(a). USGS analysis of continuous water-level data indicated that nearly 100% of the drawdown due to pumping occurred within 600 minutes. Further comments in Section 2.3.

p. 1-5(b). The capture-zone analyses are inconclusive because of incomplete hydrogeologic characterization, and have previously been questioned by GA-EPD. The three approaches to capture-zone delineation depend on the assumption that ground-water flow at Site 11 is more-or-less uniform from east to the west. If ABB-ES elects to include the capture-zone analyses, they should be qualified in such a way that a different assessment of the ground-water-flow system would not discredit the analysis. We suggest a definitive statement of the purpose and usefulness of these analyses be included when they are first introduced. That is to say, the capture-zone analyses are useful for approximating the effect of the IM Phase I GWE system on the ground-water flow system. Due to the many inherent uncertainties associated with these analyses (see section 2.3), the USGS suggests that they alone cannot be used to evaluate the actual effectiveness of the IM Phase I GWE system. Further comments in Section 2.3.

2.0 Data Assessment

2.1 Ground-Water Level Trend Analysis

p. 2-2. The discussion of the numerous hydrographs is insufficient to justify their inclusion in the report. In the following comments there are several suggestions on how specific example hydrographs can be used with appropriate annotation to strengthen various points of discussion. The hydrographs, in their entirety, could be presented in the SRFI report. Why is there more than one hydrograph for each well? Do the vertical dashed lines on the hydrograph represent pumping phases? The 20-foot scale on the ordinate (water-level axis) is too large to show accurately water-level fluctuations that typically are less than two feet during the period shown. None of the effects discussed later (tidal, recharge/discharge, and barometric), however minimal, could be discerned at this scale.

p. 2-7(a). To evaluate the barometric efficiency, it would be best to choose a time period when barometric-pressure change is likely to be the only factor influencing ground-water level fluctuations, though it is, of course, impossible to remove the effect of ground-water recession. The period chosen for this analysis coincides with large and sudden changes in pumping stress due to the start-up of the recovery wells. As a result, it would be extremely difficult to separate the unknown effects of the pumping change from the unknown effects of barometric pressure during this period.

p. 2-7(b). The discussion of change in ground-water levels in response to rainfall events needs to be substantiated with a figure including a hydrograph and rainfall data plotted on the same time scale. All points of discussion included in the text should be indicated on the figure. It is difficult to distinguish any difference between recession following ('within one to two weeks') a peak due to a rainfall event and the resumption of regional recession trend. The last sentence, beginning 'Less significant rainfall...', is even harder to understand---what is a 'more natural decline', and how can rainfall 'reestablish the regional recession trend'?

2.2 East Boundary Flow

p. 2-8. Placing a constant-head boundary at some point east of the landfill to drive ground-water flow westward is a conservative modeling approach *if and only if* it is known with certainty that contaminants originate *solely* from the isolated 'hot spots' at the western boundary of the landfill and migrate westward toward the subdivision. All three approaches used in the capture-zone analysis are based on the assumption that the natural flow of ground water is uniform from east to west across the landfill. Again, this discussion should be qualified in such a way that a different assessment of the ground-water-flow system would not discredit the analysis. The first sentence of this paragraph is a clear statement of fact that may be incorrect. It would be more accurate to begin the paragraph, 'Based on available data, it is assumed that groundwater beneath Site 11 flows westward...'.

2.3 Capture Zone Analysis

p. 2-8b. It would be helpful to reiterate that the purpose of the capture-zone analyses is to provide a preliminary evaluation of the effectiveness of the IM Phase I GWE system (see comment p. 1-3b), and to describe briefly how results of these analyses support the objectives of the IM.

p. 2-10. Figure 2-1(b) indicates a uniform ground-water gradient (uniform flow) from right to left. No consideration is given to the possibility of a recharge mound or ground-water divide.

p. 2-11. The 'length' of the capture zone, L, is questionable, especially considering the possibility of other ground-water-flow-system scenarios. Also, how were runoff and evapotranspiration determined? No values were presented or discussed. Some discussion of the measurement or estimation of these two problematic parameters is warranted because net infiltration, p, is dependent on them. Uncertainty in the values of L and p, which are both inversely proportional to the width of the capture zone, the desired result, casts doubt on the credibility of this analysis.

p. 2-16. The graph in Appendix D requires further explanation. Why are numerous points plotted on the graph at different times for the same well? If the recession factor for a given well is a constant, there is no need to plot it numerous times for the same well. Do the periods represented by the numerous points for each well actually indicate the time periods for which recession was analyzed for a given well? Also, the discussion in the text indicates that three wells were chosen as control wells, representing the three aquifer units. Were the three regional recession factors that were determined for these control wells also applied to each of the other wells in their respective unit? If so, why are different values of the recession factor presented in the graph for each well. Additionally, values of the recession factors for control wells 11-6, 11-18, and 11-11C appear to change over time on the graph for some of these wells and remain constant for others. The factor for well 11-6 changes from 0.024 to 0.032 to 0.026; for well 11-18 it appears to be a constant 0.031; and for well 11-11C from 0.028 to 0.045 to 0.035 to 0.033. What is the explanation for these variations, and why do these values not match the range given in the text? It seems the analysis of trend has been made more complicated than necessary by this graph, or, perhaps, salient points have not been adequately conveyed in the discussion. A graphical depiction of recession trends and slopes superimposed on a representative hydrograph would help clarify this discussion.

p. 2-17(a). There is not adequate justification given for the choice of April 4, 1994, water levels as the static/reference water level used to calculate drawdown, nor is there foundation for the assertion that water levels in most wells 'had not fully recovered' from, I assume, Stage 2 pumping. Stage 2 pumping totalled only about 15 gpm, including only about 6 gpm from the intermediate and deep zones. Judging from the rapid water-level responses observed at Site 11 since October 1995, when USGS recorders were installed, it is likely that the water level in any of the monitoring wells would have recovered fully in a few hours from such a small pumping stress. It would be helpful to the regulators to present a detailed hydrograph of this time period showing water-level fluctuations in one monitoring well that is close to the recovery wells in support of this hypothesis. If correct, it should be a simple matter to illustrate on such a hydrograph(s).

p. 2-17(b). USGS analysis of the continuous water-level data indicated that virtually 100% of the drawdown due to pumping occurred within 600 minutes of the initiation of pumping. Therefore, the stated time period (9,000 minutes) to reach steady-state conditions and the percentages of total drawdown due to pumping that are stated to have been achieved within the first 12 hours seem erroneous. Regardless, all stated values are presented without foundation. For these values to be convincing, they should be supported by selected example hydrographs, including appropriate annotation.

p. 2-18. It is misleading to plot the location of every monitoring well on Figure 2-2, since only a small subset of these wells were used to generate the potentiometric surface shown. Plotting each well implies that water-level measurements for each well were used in the construction of this water-level surface. Only the pertinent data should be plotted in this figure, not all the unused well symbols and IDs. If it is necessary to indicate to the reader the locations of all the monitoring wells, do so in another figure—all the unused well symbols and their IDs are only a distraction here. Also, enclosed for consideration, is a different interpretation of this potentiometric surface based on the data in the Supplemental RFI Report.

p. 2-19a. Again, a representative hydrograph could lend support to the claim that 'steady-state conditions were not established in the shallow unit'. The statement 'water levels within this unit continue to drop throughout the course of the test' seems irrelevant. In the absence of rainfall, water levels in all surficial-aquifer wells drop continuously due to natural ground-water recession. Declining water levels in the shallow unit do not necessarily indicate 'drainage...to underlying units', if all the water levels fall naturally over time.

p. 2-19b. In the middle of the page is the statement 'interpretive distance drawdown plots were generated...'. There is insufficient discussion of these distance-drawdown plots, how they were generated, and how they were used in the generation of figure 2-3. The observation wells used in the distance-drawdown plots are located in various radial directions with respect to the centers at the recovery wells. Some of these directions are away from the other recovery wells, and some of these directions are toward the other recovery wells. How is the compounded drawdown effect of the neighboring recovery wells considered in the construction of the distance-drawdown plots? How are the plots used to augment the adjusted drawdown measurements used to generate figure 2-3? The adjusted drawdown measurements should be indicated in figure 2-3. Also, something should be indicated on figure 2-3 to illustrate the connection between the distance-drawdown plots and the drawdown surface generated.

p. 2-22. The process of superimposing water-level and drawdown maps to create a new water-level surface is difficult to comprehend as described, and problematic. Firstly, figures 2-2 and 2-3, which were used to create figure 2-4, are presented without data values, making it impossible to evaluate their accuracy. A figure similar to 2-2 presented in the SRFI report does indicate 16 measured data points for the same time period; however, the data do not justify the curvature in contour lines (which are, oddly, not exactly the same as in figure 2-2 of this report). As mentioned in comment for p. 2-18 above, I will enclose in this letter another interpretation of the data. Secondly, both figures 2-2 and 2-3 are based on a relatively small number of actual measured data points (16 in the case of figure 2-2), so there is a substantial degree of uncertainty associated with the location of the contour lines. There also is substantial uncertainty related to the distance-drawdown analysis used in the construction of figure 2-3 (see comment on p. 2-19b), in addition to the uncertainty in the estimation of the corrected drawdown values (see comments on pp. 2-16, 2-17(a), 2-17(b)). Superimposing the two figures compounds all this uncertainty. The numerous 'equipotential data points' in figure 2-4 are artificial values. Thus, the statement that the process 'provides additional equipotential data for better resolution, additional equipotential data points between wells, within the area surrounding the recovery wells' is incorrect and misleading. The composite surface is subject to greater uncertainty than either of the component surfaces.

Considering all the uncertainty associated with each of the several analyses used in the construction of figure 2-4, the flow paths drawn based on the equipotential contours are of limited accuracy, and are not a convincing depiction of the flow system nor a reliable indication of the capture zone of the recovery system. Because of its limited usefulness, we suggest this analysis be removed from the report and eventually replaced by a more accurate representation of water levels and drawdown based on field measurements and not encumbered by the inaccuracies of several levels of interpretation. If it is to be retained, a thorough discussion of uncertainty should be included in the discussion of each of the several analyses that precede and form a basis for this superimposition procedure. Additionally, a summary discussion of all sources of uncertainty should be included here.

p. 2-23. The apparent data values plotted to the nearest 0.01 ft on figure 2-4 are not identified. If they are measured water levels, nine of the 13 values do not fit the contour lines. Also, the values do not agree with data presented in the SRFI report for the same time period. This figure, if it is to be retained in the report, needs substantial revision to make it legible and understandable.

p. 2-27. What is the basis for the location and elevation of the specified head boundaries at both the eastern and western boundaries? What is the basis of the configuration of the base of the aquifer? How were the various rates of recharge distributed throughout the model domain, and how were these distributions determined? Further discussion of infiltration and evapotranspiration values is warranted. Overall, this is a very cursory discussion of digital model input parameters—almost all the information in this discussion is stated in table 2-4. This discussion should provide the reader with some additional insight into how the values listed in table 2-4 were determined. As written, it provides little information that is not in table 2-4.

p. 2-32. Why is the simulated drawdown shown in figure 2-8 much greater than the observed drawdown in figure 2-3? This large discrepancy between simulated and observed values casts doubt on the credibility of the digital model analysis, and indicates poor calibration of the model under stressed conditions.

3. Conclusions and Recommendations

p. 3-1. Using values of transmissivity from aquifer tests and well construction information, USGS computed an expected specific capacity for the recovery wells. Comparing the expected to the actual specific capacity of the wells indicated a very low well efficiency.

p. 3-4. The additional three simulation scenarios of the FLOWPATH model are subject to the same criticism stated in the comments for pp. 2-27 and 2-32 above. Considering the uncertainty of boundary conditions and model parameters, and the apparent lack of model calibration, the accuracy of results from these analyses of the upgraded IM system is questionable.

p. 3-5. It is difficult to discern the locations of the recovery wells at this scale. All monitoring wells and piezometers should not be shown on this figure when the focus of the discussion is the recovery wells. Resolution could be enhanced by increasing the scale (zooming in), and displaying only the recovery wells.

p. 3-6(a). Again, it is doubtful that the FLOWPATH model is accurate enough to be used as a predictive tool. Even if it were free of the problems previously stated (see comments on p. 2-27 and p. 2-32 above), the model would still be subject to the limitations of the entire capture-zone analysis, which are discussed in the comment for p. 1-5(b) above.

p. 3-6(b). Considering the comparison of figure 2-3 and 2-8 (see comment for p. 2-32 above), figure 3-4 also most likely is erroneous and substantially overestimates drawdown.

p. 3-6(c). What is meant by the statement 'may not be hydraulically influenced by the existing wells?' It is clear from the measured drawdown maps that the pumping provides some drawdown in all the proposed well locations.

p. 3-7, p. 3-8. See comments on p. 3-5 above.

p. 3-10(a). The IM Phase I GWE system was intended to prevent migration of contaminants across the western boundary of the landfill. Pursuant to GA-EPD's statements on its ultimate objectives, the focus of future clean-up efforts may eventually be to develop techniques or procedures that will reverse ground-water flow in the Crooked River Plantation subdivision, including, but not limited to, the installation of an impermeable cap over the landfill.

p. 3-10(b). Where will the performance-monitoring wells be located? How was the number, three, determined? How were the locations determined? More than three are probably warranted given the uncertainty of the analysis of ground-water-flow directions.

I hope this review has been constructive. If you have any questions, please feel free to call.

Sincerely,

L. Elliott Jones

L. Elliott Jones
Hydrologist

Enclosures: Water-level contour map, April, 1994; annotated manuscript (without appendices).

MEMORANDUM

TO : Mr. Anthony Robinson, SOUTHNAVFACENGCOM
FROM: Sandi K. Mukherjee, NSB KINGS BAY
SUB : CORRECTIVE ACTION PLAN FOR PHASE II INTERIM MEASURE
DATE: 09 Feb 96

1. Reviewed the DRAFT subject report and I have several comments which may require further consideration. If SOUTHDIV AND USGS concur with these comments, suggest ABB incorporate these comments in the draft final which is due to GA EPD 15 Mar 96.

2. Suggest revising title of the report to "CORRECTIVE ACTION PLAN (INTERIM MEASURE SYSTEM UPGRADES) in place of "CORRECTIVE ACTION PLAN FOR PHASE II INTERIM MEASURE". The latter title, besides being more complicated (with the Phase II term thrown in), implies we are still continuing in the interim measure phase which in reality we are not. My contention is that we are out of the IM phase and have progressed to the CAP phase. Lets delete references to IM Phase II and instead emphasize CAP in the text.

3. Page 1-4, Section 1.2: Suggest inserting the word "suspected" in line 8 of the text to read " Two suspected areas where flow of contaminated groundwater ...., and north of RW-5". There is NO evidence that slip through is taking place between RW-3 and RW-4 and north of RW-5. Neither is there evidence that this IS happening unless we believe the simulated capture zone (fig 2-3 on page 2-5) accurately depicts IM system performance. Mike Maughon (SOUTHDIV Tech Support) does not believe the CZ modeling has been done accurately and consequently inserting the word "suspected" may be appropriate.

4. Page 1-4, Section 1.2, Line 9: The statement "Performance criteria for air emmissions are not being met" is not accurate. We have been meeting the agreed upon 370 ppb criteria but we are not meeting the newly imposed criteria of background/zero emissions. The statement should be reworded to reflect this controversy.

5. Page 1-4, Section 1.3: This section should be revised to reflect a phased approach to the implementation of the CAP. Specifically, the approach should follow a logical sequence starting with re-evaluation of the present system using groundwater modeling; redevelopment of the existing recovery wells to assess present system output limits; a concurrent effort to determine site-specific natural attenuation possibilities; installation of a recovery well in the hot spot area around MW-13; installation of additional recovery wells if necessary,etc. The Scope as described in the subject plan does not reflect the approach suggested above.

6. Page 2-1, First Sentence: Suggest revising the introductory sentence to read " This CAP for VOC- and SVOC-contaminated groundwater at Site 11 entails continued operation of the

Encl(2)

existing GWE and treatment system with the upgrades outlined in Section 1.3". Again, I am confused whether we are proposing an interim measure in this CAP or are we proposing a CAP per se?

7. Page 2-1, Intro Para, Line 7: In accordance with Comments 1 and 6, suggest deleting "interim measure" in this line/sentence. In fact, I would suggest we scan the entire report to reflect this perspective.

8. Page 2-4, Figure 2-2: Can we re-title this figure to something like "Schematic Diagram of Groundwater Extraction and Treatment System"? Delete words like "conceptual".

9. Page 2-5, Figure 2-3: Can we remove this figure from the text? Even Mr. Willard Murray said this should not have been included in the E&R report! See Comment 3 above.

10. Finally, there are no figure(s) depicting the plume in this CAP document. Can we have plan and section views to show horizontal and vertical extent of contamination? I know we have data to generate these figures and there are some that have been produced in the past.